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BEHAVIOR ANALYSIS AND SOCIAL PSYCHOLOGY: A REVIEW OF LANA'S ASSUMPTIONS OF SOCIAL PSYCHOLOGY[1]

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There are several reasons why this book is worth reviewing for *JEAB* readers. First, it is one of the few books in social psychology to take the behavior-analytic approach seriously. Whereas most social psychologists are familiar only with neo-Hullian and associative learning approaches to social behavior (S. Berger & Lambert, 1968; Lott & Lott, 1985; Staats, 1975), Lana's (1991) book has a complete chapter on "Skinner and the Behavior Analysts." A second reason is that the book is about the assumptions of social psychology, rather than about its experimental findings. This provides a good chance to compare the behavior-analytic foundations for social behavior to those of social psychology, and help find out why behavior analysis has had almost no impact on the latter. Finally, one of Lana's goals in his book is to examine "if or how social psychology can be an experimental science" (Lana, 1991, p. vii). A reviewer can therefore ask how behavior analysis is progressing towards providing answers to Lana's question. What are the problems with an experimental approach to social behavior, and are there social phenomena that we are not addressing at present?

Overall, Lana's book reviews the progress of social psychology in the last 20 years. Lana believes that there are social phenomena that are missed by both recent social psychologists and behavior analysts. Although I shall argue that Lana's characterization of behavior analysis sometimes misses the mark, mainly when addressing causality, intentionality, and verbal behavior, his book is important for both behavior analysts and traditional social psychologists because it shows some of the ways the subject matter of social psychology is changing. All those interested in social behavior need to catch up with these developments.

Social Psychology and Behavior Analysis

Even a cursory glance at social psychology textbooks or major monographs will reveal that behavior analysis has had little or no impact on social psychology. Most of the arguments used against behavior analysis in such books are ones that misunderstand behavior analysis; these also figure elsewhere in psychology (Todd & Morris, 1983). Other arguments, sometimes by key social psychologists in key books, are pure rhetoric: "Bem's initial theoretical statements stemmed from a Skinnerian radical behaviorist perspective involving a somewhat mysterious language of 'mands' and 'tacts'—terminology that has befuddled many a psychology major" (Fazio, 1987, p. 129).

The most recent *Handbook of Social Psychology* has only a few scattered references to Skinner and no references to the research on social behavior by Hake, Schmitt, or others within behavior analysis (Lindzey & Aronson, 1985). The Skinner references are almost all misinterpretations of behavior-analytic positions, and the chapter on "learning" is neobehaviorist in flavor, based on Hull and drive theories (Lott & Lott, 1985). Beyond the superficial and erroneous, only the chapter by Weick on systematic observational methods gives a nice introductory quote from Skinner's *Notebooks*.

The gist of discussions of behavior analysis in social psychology can be seen in quotes such as the following:

> The major reference experiments of Thorndike, Pavlov, Guthrie, Hull, Tolman, and Skinner

[1] Lana, R. E. (1991). *Assumptions of social psychology: A reexamination.* Hillsdale, NJ: Erlbaum.

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> involved animals interacting with various features of the physical environment. This environment could be described objectively in terms of centimeters, grams, or seconds. The everyday social behavior of human beings, of course, defies description in these terms. To extend the S-R analysis to such behavior episodes requires reference to some internal set of mechanisms encompassing attention, perception, memory, and complex cognitive transformations. (Jones, 1985, p. 72)

The same sort of argument is echoed by Lana as a general problem for an experimental approach in social psychology:

> Difficulty arose when some of those uncontrolled variables that were thought to vary randomly were responses from the subject directed toward being in the experiment itself. In short, by *interpreting* various aspects of his or her participation in an experiment, *the subject* introduced a confounding variable that rendered the results of the study either meaningless or grossly distorted." (p. 3, my italics)

So we are dealing with a situation in which social psychologists know little about behavior analysis and its assumptions. It should be noted in this context, therefore, that despite the faults I will find in Lana's presentation of behavior analysis, he has done a better job of understanding and presenting it than have almost all other social psychologists.

THE VICISSITUDES AND CRISES OF SOCIAL PSYCHOLOGY

Arguments such as those above were partly behind the rise of cognitivist social psychology, which has been the dominant viewpoint since the 1970s. "Social psychology and cognitive social psychology are today nearly synonymous. The cognitive approach is now clearly the dominant approach among social psychologists, having virtually no competitors" (Markus & Zajonc, 1985, p. 137). The gist was that people do not react blindly to stimuli like animals do, but rather, they have *perceptions, representations,* or *interpretations* (see quote earlier) of a setting to which they react instead. Therefore, it was argued, to predict the behavior of humans, social psychologists needed to study how people think about or perceive the environment around them, rather than to specify the environment itself in great detail. As is well known to behavior analysts, such arguments merely relocate stimulus control and a history of stimulus control into the organism's body in the degraded form of a "schema," a "mental representation," or a "script." But for social psychologists these were influential arguments that put the focus onto how thinking determines behavior.

Although Lana sometimes seems sympathetic to such arguments (e.g., p. 100), his main argument is more than this, as we shall see. Apropos the quote above from Markus and Zajonc, Lana is reacting to a more recent development, which should be of interest to behavior analysts: that the influence of the cognitivist approach is waning in social psychology. The thrust of Lana's book is to examine what has gone wrong with the cognitive approach and a consideration of what should replace it. To his credit, then, Lana seriously considers behavior analysis as one viable option.

Critics of Cognitive Social Psychology

Affect. Arguments against the cognitive approach in social psychology have arisen in several quarters. In a historical chapter, Jones (1985) restated the first of these, suggesting that "affect" might come to dominate cognition as the principal explanation of social behavior. Zajonc (1980), for example, had argued strongly that affect and cognition are independent response systems, and that people can have preferential (or differential) feelings towards stimuli without having thought about the stimuli. The dominant position since Schachter and Singer (1962), though, has been that there are only very general changes in feelings that can be *interpreted* differently by thinking about them, so affect is determined by how we think.

Before throwing a baby out with all this verbal bathwater, the events or phenomena that are being talked about as affect should be examined. The basic position is that there are activities and stimuli that we engage in or verbally prefer. The strongly cognitivist position of Schachter and Singer (1962) was that the activities and stimuli we prefer are determined by what we think about them—how we talk to ourselves and others about them. Any physiological changes that might

occur (as was measured during the time of drive theories) are also determined by thinking. Contrasting with this, Zajonc's (1980) position was that "preferences need no inferences," that like nonhumans, we can respond differentially to stimuli and engage more frequently in some activities without the occurrence of thinking, although thinking can change preferences under some conditions.

It should be clear from this rewording that the term "affect" is used in a very loose and general way by social psychologists and most often means doing one activity rather than another. Put this way, Zajonc (1980) is stating a position with which behavior analysts should have no problem (putting aside the words used): first, that stimuli and consequences can control behavior without the occurrence of any verbal behavior specifying those contingencies, and second, that verbal behavior can sometimes control nonverbal behavior if there are contingencies for this (Hayes, 1989). Although Zajonc does not cite it, the classic Hefferline and Keenan (1963) study would be evidence for his position (see also Zajonc & Markus, 1984, where the locus of preferences is shifted from cognitions into motor movements, in a very Watsonian fashion).

What social psychologists are getting at, then, by talking about affect, are the motivators for social behavior. The cognitivist assumption that thinking is necessary to make behavior happen is no longer appealing to social psychology, so affect is suggested as a general motivator of behavior (very reminiscent of drive). From the behavior-analytic position, affect is being used to name discriminative stimulus control but without reference to the reinforcement contingencies that make stimuli operative. This is perhaps understandable, because the evidence presented for such views involves contingencies with very subtle, automatic (Vaughan & Michael, 1982) consequences, probably generalized and involving other people (e.g., Kunst-Wilson & Zajonc, 1980; Tassinary, Orr, Wolford, Napps, & Lanzetta, 1984). What is being looked at, then, in the affect literature, are the ways in which verbal behavior (cognition) and nonverbal behavior (under the rubric of affect) influence each other. But because the cognitivist approach has given priority in the past to verbal behavior controlling other behavior, social psychologists are now trying to show that behavior can occur without verbal mediation. This, effectively, is a struggle towards the point at which behavior analysis begins.

A similar approach to affect has been to talk about "hot," "cold," and even "warm" cognitions (Sorrentino & Higgins, 1986). In behavior-analytic terms, cold cognitions are the verbal behavior that governs other behavior, and hot cognitions are behavior that is determined by nonverbal contingencies rather than by verbal rules (so hot cognitions are not really cognitions in the first place). Once again, the crucial distinction that social psychologists are trying to make is between verbally governed behavior (cold cognition) and behavior directly contacting the contingencies specified only in verbal rules.

The self. A second way of avoiding pure cognition in social psychology has been the emphasis on theories of "self." Not unexpectedly, from what has been said so far, this has gone hand in hand with the development of theories of affect and motivation (Sorrentino & Higgins, 1986). There are several ways "self" has been used, but the primary function has been as a theoretical home (read as "noun") for an individual's accumulated experience. Originally, this development in social psychology was a cognitivist one, that the self consisted of structured thinking and the storage of information (read as "verbal behavior with generalized social consequences") about the self (Markus, 1977). More recently, the self has included affect and other hot cognitions as well.

The position of self theories for behavior analysts is perhaps best clarified in Wicklund's (1986) suggestion that the self has two orientations (read as "theoretical uses for psychologists"). The *dynamic orientation* of the self is a fit between the "demands of the environment and the individual's perceptual and behavioral readiness" (p. 65). This requires no thinking (verbal behavior); indeed, thought would merely interrupt the dynamic interplay between action and environment. For behavior analysts, this orientation names the nonverbal contingencies contacted by a person as well as the person's previous history of contact with contingencies. If it were not for the history part, Wicklund would probably

not locate this self orientation within the body at all, because the dynamic orientation includes both the action and the environment. But because the dynamic fit depends upon a person's personal history, this part of the self is thought to reside in the person.

The second orientation of the self is the *static orientation* (Wicklund, 1986), in which the individual comes to name and verbalize about the environment and his or her own dynamic fit to the environment. This is the concern of most social psychologists and especially the cognitivist approach to the self: How do people talk about their own behavior, and what effect does this have on behavior? Wicklund contrasts acting in the environment to talking about the environment. (Wicklund also draws a point that is pertinent to the behavior-analytic account: the *function* of the static orientation. He rightly argues that we need to consider what is gained by verbally specifying the environment rather than directly acting on the environment specified.)

So once again, the social psychological alternative to a purely cognitivist approach leads us to relegate the cognitive effects to verbal behavior and an unspecified basis to the noncognitive effects. In Wicklund's (1986) case, the latter is called dynamic orientation rather than affect. Although the self approach has one behavior-analytic advantage over the affect approach, in that there is a role for a personal history of contingencies, its locating the self in the body at the same time downplays stimulus control.

Social is not in the head. A third approach to criticizing the cognitivist influence in social psychology has come from those concerned with "putting the social back into social psychology." This view, especially important in European social psychology (Forgas, 1981; Moscovici, 1972), criticizes the individualism of cognitivist social psychology and the placement of all social psychological phemonena inside the head. Those arguing this position disagree that social and societal effects have an influence only by an individual schematizing and cognizing about social groups, internalizing the output as representations, and then responding on the basis of their individual cognitive schemata.

> I . . . suggested to my audience that for them the social was located in the individual, that is, in his or her mind and perceptions, while I located it outside the individual, in the external environment. For them, representations signified "cognitions in the head," and not "between our heads," in language and communication. (Moscovici, 1990, p. 66)

In different ways, these critics wish to give a more social role to social phenomena, and to this extent they go beyond the cognitive approach (Farr & Moscovici, 1984). As argued elsewhere, behavior analysis can provide a basis for this by showing the *essential* role of other people in verbal behavior, and hence in "language and communication" (Guerin, in press; see also Blackman, 1991).

Nonobjective social realities. The final alternative to the cognitivist influence in social psychology is the one for which Lana argues, and which forms the basis of his book examining the assumptions of social psychology. Lana argues that the subject matter of social psychology lies in a nonobjective reality that has not been appreciated in social psychological theories.

> Social psychological reality is a product not only of measurable objective conditions surrounding the lives of several individuals, but of the relationships and agreements made among people living together. These relationships and agreements are embodied within the laws, customs, folkways, and language that are shared among people in the group. This context constitutes a reality, a condition of existence, which is as important to understand as the objective conditions in which the group finds itself. . . . *This context is not amenable to causal or contingency analysis for the same reasons that some human decisions are not; it involves intentional responses and is a product of the unique history of the group.* (p. 15, my italics)

The last sentence of this quote contains much of Lana's book, that "intentional responses" are different in kind to the responses produced by the rest of the animal kingdom, and that unique histories make lawful social psychology impossible. Before outlining these arguments, however, it is worth mentioning an influential approach similar to that of Lana.

Gergen (1988, 1989; Gergen & Semin, 1990) has argued that the cognitivist revolution in social psychology has become untenable, and gives some of the arguments already mentioned above. Instead of the cognitivist approach, Gergen suggests that we

> shift the point of concentration from mind and world to the relationship between words and

> world. Let us shift attention from "the propositions in our heads" to the propositions in our written and spoken language. In this case we may put aside murky questions about how schemas, prototypes, memories, motives and the like operate, and focus on the way our words are imbedded within our life practices. (Gergen, 1989, p. 471)

The move suggested by Gergen has some points in common with behavior analysis. In particular, taking verbal behavior out of the head and not treating it as a store of mental representations is a positive step, as is locating it in "life practices" that involve other people (a verbal community). The problem, however, comes with the distinction between verbal and nonverbal behavior, in that Gergen seems to ignore nonverbal behavior completely. By his arguments, social or verbal knowledge is to be considered a separate discipline from the rest of psychology (elsewhere he calls it part of the "interpretive disciplines," Gergen, 1985, p. 270). The other critics of cognitivist social psychology, referred to earlier in this paper, at least provided a name for nonverbal responding (affect, dynamic orientation). The point of Gergen's theoretical move, then, seems to be to get the stuff of social psychology out of the head by putting it into a particular group of practices or actions: those concerned with reading, writing, and communicating generally (see also Hanks, 1989). He especially emphasizes that this must occur in the interactions between people, and not as an individual action based on a representation of another person (Gergen, 1988).

THE ARGUMENTS OF LANA

It has been argued so far that social psychology has changed dramatically in the last few years and that new assumptions are being explored that are less at odds with behavior analysis than were the comparable assumptions in recent decades. Lana's book is part of the same changing process, which he explicitly covers in his early chapters. The main arguments of his book then work towards the conclusion that a further *historicosocial* perspective is needed in social psychology, along with the behavior-analytic and cognitive approaches:

> The unique assumption of the historicosocial position is that many behaviors called social cannot be reduced to the theoretical entities that we use to describe individual behavior. . . . That is, human beings as much create the socius as they are formed by it. . . . As is the case with cognitive theorists, it is not expected that functional, predictive laws will be developed from such a description. (Lana, 1991, p. 133)

I will examine this historicosocial perspective after reviewing briefly the arguments leading Lana to such a position.

Causation

Lana develops an argument that the functional accounts of behavior analysis are causal, and that there are perspectives in social psychology that cannot be causal because of human intentionality and choice. Although I find many parts of this argument confusing, some flavor of it will be given.

Lana rightly points out that behavior analysis is not about mechanistic causes (Todd & Morris, 1983), but later he seems to contradict this: "Cause as a metaphor can be applied only to events that can be objectively perceived, because events in a cause and effect sequence require that each be identified as a discrete entity" (Lana, 1991, p. 30). What this misses is Skinner's (1935; see also Lee, 1981) proposal to view an operant as a *class* of responses. By this alone, the behavior-analytic view is neither a mechanistic nor a causal one in Lana's sense, because event instances can be "objectively perceived" but the class of responses in a contingency cannot.

The main point that seems confused in this and other discussions of causality is that causality is a form of verbal behavior—causality arises only when we talk about events in the world. Causality is a form of talking such as "If I turn the door handle and push, this will cause the door to open." This is very different from the behavior of a cat that has learned to open doors by turning the door handle. The philosophizing about causes should therefore involve *knowing that* (Ryle, 1949) rather than *knowing how* (Hineline, 1983). This distinction is important because when we read statements such as "For Kant, the actual nature of the objective world was unknowable" (Lana, 1991, p. 49), we are only dealing with *knowing that.* In the sense of *knowing how,* the cat clearly "knows" the objective world.

The same point of confusion continues through the arguments presented by Lana

(and in this, he is not alone among psychologists and philosophers). It is important for behavior analysts because the two types of knowing imply two types of contingencies. An organism knows (*how*) the sun rises if the sun functions as a discriminative stimulus. Responding differentially when the sun is up and down is evidence of directly knowing the sun rises. Roosters do this very well, and Gibson (1979) and Skinner (1974; see also Guerin, 1990) were careful to make the same point about direct knowing. But truth and meaning are usually meant to apply to *knowing that,* for example, the truth value of saying "The sun always rises in the morning." Given that this is now about verbal behavior, the classical philosophers were correct that we can never be certain about such statements (Descartes), that there is always historical contingency in finding causality (Hume), and that it all depends upon our previous knowledge and use of language forms (Kant). The difference, however, is that the truth or meaning (of *knowing that*) they are talking about is assumed to be a *correspondence* between the contingencies specified by the verbal behavior and the occurrence of those contingencies in the objective world of the rooster. And as behavior analysts we know that exact correspondence is often lacking between our own verbal behavior and nonverbal behavior, let alone between our verbal behavior and the objective world.

The behavior analysts' solution, closely following Dewey and Bentley (1949) and other pragmatists, is that the truth and meaning of verbal behavior lie not in their correspondence to the world but in their effects, and the effects of verbal behavior occur primarily in the behavior of other people. So the truth or meaning of saying "The sun always rises in the morning" is the effect this has upon other people. In this sense, such verbal repertoires are maintained, and thus are "true" only to the extent that they get someone else (another scientist in the case of scientific statements) to do something. "That is a cat" is true only in the sense that saying it functions to change the behavior of someone else and strengthen a class of verbal responses.

Let me give another example of the distinction being made. In this case, the verbal–nonverbal distinction is mixed up in the word "perceive."

> Perception cannot be meaningfully described probabilistically or the perceiver would not experience the perceptual stability he or she does. . . . [T]he Muller-Lyer illusion lines are neither equal nor unequal. It is only in the objective world that this question arises. [I]t is clear that the linear mensurative system is not appropriate for building expectations regarding perceptual reality. (Lana, 1991, p. 74)

"Perception" is a tricky word in cognitivist and social psychology because it vacillates rapidly between *knowing that* and *knowing how*[2]. If we separate the two, then perception can in fact be "described probabilistically" (verbal) *and* still the perceiver "experiences the perceptual stability" (nonverbal). There should be no problem with this because two different types of behavior are involved. The linear mensurative system is not used in building expectations (verbal) regarding perceptual reality (nonverbal), but is used only when comparing your nonverbal behavior with that of someone who has heard you talk about your verbal expectations ("Does that look three inches to you?").

So the important difference lies between contacting contingencies (experiencing) and talking about those contingencies (describing or providing expectations). Both are controlled by discriminative stimuli and consequences, but these are very different for the different classes of behavior. We can know the objective reality of how to ride a bike without being able to talk about it. Conversely, someone can say "E = MC^2" to us but no behavior appropriate (which reinforces the speaker's verbal behavior) to the truth of this statement is occasioned.

The point made in the preceding paragraphs highlights a crucial flaw in Lana's argument, because if other people are left out at this stage of the analysis, by assuming

[2] It is ironic, therefore, that Lana later writes that "behavioral systems from Watson's to Skinner's have not addressed themselves to the nature of perception. . . . [They] have little to say about traditional perception because it is a phenomenon immediately experienced by the organism. . . . Perception [is not a] characteristic that can be changed by reinforcement" (p. 78). The major role of stimulus control in contingencies has obviously been missed ("immediately experienced"), as has the large amount of research on changing perception by reinforcement.

a correspondence theory of truth rather than a (social) functional theory, then they cannot reappear later as the mediators of intentionality and social realities. By imputing a nonfunctional theory of truth onto behavior analysts, Lana has already assured his later points that intentional verbal statements cannot be causal or functional, and that there is a social reality which behavior-analytic statements cannot capture. What is ironic in all this is that Lana denies this as a distinction within behavior analysis (e.g., in the quote about perception above) and then uses it as a reason why behavior analysis is not sufficient—because it is not acknowledged that behavior analysis differentiates between social and nonsocial reality.

The crucial point for the behavior analyst's position is to distinguish clearly the types of behavior occurring and those being specified by verbal behavior, and the contingencies that maintain both of these. Within this analysis, social contingencies are not different in kind from nonsocial contingencies. We know (how) objects through contacting the effects they have, and we can also know (that) these objects by verbally specifying their contingencies—which only means that we have a particular effect on a person by saying them. We can also know (how) social contingencies by contacting them (when we interact with other people and have mutual effects), and we can also know (that) these (social) objects by specifying their contingencies verbally (talking about how other people react, making attributions, expressing attitudes). As a subset of the latter, we can know (how) the *verbal* social contingencies merely by talking successfully to other people in a verbal community, and we can also know (that) the verbal social contingencies by talking about talking to people.

As emphasized above, the contingencies for the last two events (talking successfully, and talking about talking successfully) are likely to be distinct. We have already seen that there is no reason why successfully riding a bike should have the same occasioning and consequences as talking about how you ride a bike. *In the same way, when applied to verbal behavior instead of bike riding, there is no reason why successfully talking in a verbal community should have the same occasioning and consequences as talking about your talking to a verbal community—as a social scientist might.* But if the truth criterion is correspondence between words and an objective (nonverbal) world, then it leads one to the position (which Lana argues) that one cannot be both a scientist observer of social behavior and at the same time participate in the verbal community being studied. The argument is that talking successfully and talking about talking successfully cannot be analyzed in the same way. The way to study social/verbal behavior, therefore, is to participate, experience, self-reflect, or understand the "concrete lived reality" (p. 104).

This line of reasoning leads Lana (and Gergen) into hermeneutics and phenomenology (see also Day, 1969, on phenomenology). It can be seen that from the behavior analyst's point of view, however, that just because talking successfully and talking about talking successfully have different stimulus controls and consequences does not mean that they cannot be analyzed in the same way by the same person. *Knowing how* to talk in a community and *knowing that* about talking in a community will have different types of contingencies, but they can both be functionally analyzed in terms of contingencies. As I have tried to argue, it is the hidden assumption of a correspondence theory of truth (applied to verbal behavior or *knowing that*) that leads Lana to assume that behavior analysis cannot encompass both talking in a community and the talking about talking in a community that constitutes much of social scientific knowledge.

Choice and Intentional Responses

In addition to causation, Lana looks at intentions as another stumbling block for social psychological explanations. Here too, he fails to take a thoroughly functional position:

> A problem with the contingency explanation of intentional behavior by the behavior analysts [is] that an individual can always choose to do exactly opposite of what he or she has habitually done in the past simply by so deciding. Also, knowing that someone has predicted one's behavior can result in he or she not behaving according to the prediction. Although these objections do not damage the contingency interpretation of intentional behavior in most situations, they do represent limits to its application. (p. 70)

The point has been lost here that the choice and "not behaving according to the prediction" also have stimulus controls and consequences; these might be distinct from the stimulus control and consequences that produced the intentional statement in the first place. Lana's argument (p. 70) that this is tautological (but again, tautologies exist only in verbal behavior) was a point Skinner solved by requiring that reinforcement be demonstrated (nonverbal) rather than inferred verbally (Skinner, 1938). Producing intentional statements might have all manner of stimulus controls and consequences that have nothing to do with the intended behavior specified. Just as saying "The sun will rise tomorrow" involves events different from those that actually keep the earth revolving, so, too, saying "I will rise at 5:00 a.m. tomorrow" has controlling variables different from those that directly affect getting up the next morning. The production of intentional statements must be studied separately from the events they specify, even if the same person makes them and is specified by them.

Finally, with the arguments about intentions, Lana again draws the conclusion that the proper study of social psychology should be separated from the law-making of the natural or physical sciences: "The possibility for choice that is always present in human beings precludes achieving in social psychology the success enjoyed by the natural sciences" (p. 10). If his premise were true, the stated conclusion would follow, but I have argued that behavior analysts would not agree with the premise that unanalyzable choice occurs with humans.

Cognition

Although he concentrates on behavior analysis and hermeneutics as the major alternatives in social psychology, Lana still argues a place for cognition. He makes cognition out to be the study of the structure of thought and hence the brain. For example, it is claimed that Chomsky mapped out the structure of grammar (which Skinner largely ignored) and described its operation by its form (Lana, 1991, p. 91). Behavior analysts do need to pay more attention to the structure of behavior repertoires, but it is the locus of the structure that is really in question here, not whether there are patterns in behavior or not. Lana seems to go along with the cognitivists in asserting that brain structures map into cognitive or thinking structures (e.g., Lana, 1991, Figure 7.1).

For behavior analysts, the structure of behavior, and even of thought, lies in the patterns of interaction between behavior and environmental events. The structures that are found, whether cognitive or social, are functions (contingencies) that have been repeated and have become, as it were, ossified. But the control of repetition still lies with discriminative stimuli and consequences in contingent relations, rather than with an autonomous agent. The contingencies might arise through stable practices of a verbal community, through an individual's reinforcement history with a limited range of contingencies, or through the very structure of the environment. As an example of this third type, we always open the door of a refrigerator before getting anything out, so the two activities form a repeated, integral unit and therefore a structure. The control of this does not lie with a verbal community's arbitrary pronouncement about how to open refrigerator doors in this case, but with a basic fact (a setting event) about the environment—that we cannot take something out of a refrigerator if the door is closed.

To give another example of structure, let us consider grammar, the example used by Lana (p. 91). Clearly, in any sense of the term, there is structure in grammar. Words are divided into units that have rules (although sometimes these are broken), and certain orders of words are used as well as certain special forms, such as putting "s" on words for plurals. Where, though, is this structure or the control of the structure? Is it in the head, or is it an innate linguistic acquisition device? This example is nice because the argument from behavior analysis is that the structure of grammar is in the social/verbal community rather than in the head, and this is the very sort of social event Lana and Gergen wish to make independent of individual functioning in their new approaches.

To show this, consider the following. If grammar persists in a verbal community, it presumably must have a function. A likely candidate for this is simply *ease of verbal production*. If there were no grammar, we would run all our words together and not

distinguish between plural and other forms. In such a case, the reinforcing and punishing effects of speaking would be greatly diminished. Similarly, if I were to write the following it would be harder for readers to act upon: "SimilarlyifIweretowritethefollowingitwouldbeharderforreaderstoactupon." So grammar, in this way, can be seen as functional insofar as it makes the production of verbal behavior easier and the production of its effects easier. Grammar does not make a listener act upon the environment, like a tact or a mand, but facilitates the effectiveness of the verbal behavior. This is almost the definition of an autoclitic (Skinner, 1957).

What follows from this is that the locus of grammatical structure is in the practices of a verbal community rather than in the head of the speaker or listener. Grammar is maintained by a verbal community and is an essentially social phenomenon. So what Chomsky and transformational grammar have done is to map out commonly repeated (they say universal) contingencies with which the production and effectiveness of verbal behavior are facilitated. For example, verbal behavior is made more effective by separating the behavior into smaller units or words that have breaks between them. This does not tell us anything about the structure of the brain and its operation, but about the structured practices, and operation of a verbal community.

Lana's Account of Verbal Behavior

Several of the arguments so far have hinged upon the role of verbal behavior. Lana deals with this in his chapter on Skinner and behavior analysts, and correctly sees verbal behavior as pivotal, whereas most social psychologists see this as Skinner's weakest point. On the whole, Lana presents the basic behavior-analytic account of verbal behavior accurately and effectively. Also appropriately, he focuses upon autoclitics as important to his key issues. But as indicated in the previous section, I believe that he misinterprets autoclitics in an important way. Consider this quote:

> Even with this nonexperimental approach to language, Skinner clearly intended that the form of his explanation be no different than that offered regarding schedules of reinforcements discovered in animals. . . . If the autoclitic system is one in which some verbal behavior reinforces other verbal behavior, the causal chain suggested by events external to the organism reinforcing behavior is broken. . . .However, because the autoclitic is a concept that describes behavior (i.e., behavior influencing behavior) it involves only verbal internal processes and therefore is different from those explanations of behavior when the reinforcement is clearly external to the organism. (p. 92)

As I argued in the previous section, the locus of the autoclitic is in the verbal community rather than "verbal internal processes." What Lana suggests is a mistaken idea of reinforcement, similar to the idea that self-reinforcement is an internal reinforcement within the person rather than a form of social reinforcement. Autoclitics, such as "Please . . . ," grammatical endings of tense and case, word orderings, "I am reminded that . . . ," and "It is true . . . ," work or function only through the mediation of other persons. They cannot work as internal functions ("some verbal behavior reinforces other verbal behavior") but have their effect on other members of a verbal community. If there were no other people we would not use the phrase "Please . . ." at all.

Most of Lana's arguments against behavior analysis in this section of the book rest on this mistaken view of the locus of autoclitics. The other mistake (of emphasis) lies in the first sentence quoted above, that Skinner intended his verbal behavior account to be no different from other schedules of reinforcement. Although this is strictly true in *principle,* it is worth being reminded that "Behavior which is effective only through the mediation of other persons has so many distinguishing dynamic and topographical properties that a special treatment is justified and, indeed, demanded" (Skinner, 1957, p. 2). This will come up shortly when considering the details of a behavior-analytic account of historicosocial analysis.

Social Behavior

Lana also deals with Skinner's (1953) account of social behavior and focuses his arguments on the system of laws and documented social rules. He asserts that such historical matters as laws are difficult for behavior analysts to deal with because the contingencies that produced them are perhaps 200 years

old. This, however, confuses two issues. First, the contingencies that have people following those laws and codified rules are not 200 years old but are present here and now. The same is true of natural selection: The forces that today keep the giraffe's neck functional need not be identical with those that shaped the neck originally.

Second, behavior analysts do recognize that part of their science has to be reconstructive and historical, especially when talking about humans (e.g., Donahoe & Palmer, 1989). It is no surprise that "social psychology may be fundamentally historical in nature, classifying unrepeated and unrepeatable social episodes" (p. 10). This also occurs in natural sciences such as ecology and geology. To deal with this situation, behavior analysts have begun to look at larger units of social contingencies (Glenn, 1985, 1986, 1988), at ways of controlling for personal histories (Chase, 1988; Freeman & Lattal, 1992; Harzem, 1984; Wanchisen, 1990; Wanchisen & Tatham, 1991), at the analysis of non-Western social groups (Glenn, 1988; Guerin, 1992; Malott, 1988), and, from the beginning (Skinner, 1935), at operants as classes of responses rather than instances of responses. Lana is correct, however, that more of this needs to be done in the study of human social behavior.

Another argument comes out of Lana's discussion of social behavior. This is that the study of historicosocial (he also calls it sociohistorical) belief systems can be done apart from a behavior analysis of how they function. This parallels the distinction made by Saussure (1983) between *la langue* and *parole*. The former is the system of signs that a community might use for its language, such as the English language or a sign language, whereas the latter is the actual production of speech and writing. In this sense the historicosocial approach would be to study the patterns of development in a verbal community, how the words change and are modified, how grammars slowly change, how different parts of the body are used to make the same functional gestures at different times, and how historical events influence the shifts between different languages. The behavior-analytic approach, on the other hand, normally concentrates on how the words and signs function and the effects they have on others when emitted at any particular time. The two are not incompatible, as Lana notes, but little has been done by behavior analysts on the study of verbal communities, the changes in signs used, and historical relations between languages.

Hermeneutics, Rhetoric, Historicosocial Analysis: The Question of Social Realities

One of Lana's points is that there is a type of social reality not captured by the individualistic cognitive social psychology, nor by behavior analysis (P. Berger & Luckmann, 1967; cf. Blackman, 1991). As mentioned earlier, others such as Gergen (1988) have also taken this position. In the previous sections of this paper I have presented a case that Lana's arguments against behavior analysis are flawed, thus challenging his conclusion that behavior analysis cannot deal with social realities. It remains to be shown how behavior analysis might cope in talking about particular social realities, however. Before addressing this, some of Lana's views about the alternatives will be discussed.

The starting point for Lana at this stage of the book (chapter 6) follows from his arguments about causality and intentionality. There are stimulus controls on people from the environment, but there are also "nonreducible states of the organism that function independently of externally observable factors" (p. 100). Humans are therefore "both active and creative in their social perceptions and their consequences, which yields behavior, in part unaffected by behavioral contingencies" (p. 101). To support this, Lana appeals to three types of approaches.

Dialectics. The term *dialectic* has come to have several meanings. Lana separates three of these, and gives greatest importance to dialectic as a form of reasoning that is contrasted with demonstrative logic. Rather than deducing propositions from known truths, as the hypothetico-deductive model is supposed to do, dialectic logic works by supposing one idea and immediately considering it in relation to its opposite. He proposes that because both alternatives are considered, the environment cannot determine the dialectic process, and therefore the active organism must do the choosing. This differs from hypothetico-deductive reasoning, which is assumed to require that the environment determines the initial true propositions from which deductions are

made. With this sort of argument, Lana makes a case for an active social organism that dialectically determines its own behavior, so that social thought is described as independent of the environment.

Hermeneutics. Hermeneutics refers to a variety of procedures for understanding texts and social history. If the linguistic and other signs of culture are independent of the environment, then we should be able to interpret and understand them by examining the relations between them.

> People offer signs of their own existence. To understand these signs is to understand human social activity. The signs are linguistic and gestural and occur within a sociohistorical context. These signs, in turn, yield forms in stable configurations such as evaluations, feelings, and decisions that can be deciphered by others. (Lana, 1991, p. 103)

> Interpretations developed from hermeneutic analysis reverse the process of science by attempting to grasp the intentional quality of individual or group life experiences. These intentions need to be expressed in the language of the actors themselves rather than in a specialized language of the abstract. . . . Hermeneutics can thus be said to be a methodical approach to the everyday understanding of oneself and others in the communicative experience. (p. 104)

What is being said is that people run their lives by having goals and intentions that involve other people ("communicative experience"), and in order to understand properly this "concrete lived reality," the psychological analyst must use everyday understanding rather than replace it with a specialized language.

Put this way, we can see where the flaw in the argument arises. It was argued earlier that intentionality is kept as a key concept by rejecting the social basis for private verbal behavior and by assuming a correspondence theory of truth. If we accept instead the behavior-analytic view that the everyday understanding is available only through a verbal community, and a pragmatic theory of truth is part of this, then it is possible to study everyday understanding in behavior-analytic terms because everyday understanding is an event in a verbal community and not in an individual. The key point of a pragmatic theory of truth is that the truth of everyday understanding lies not in a presumed "objective," nonsocial environment, nor in the individual as hermeneutics would also have it, but in the shared interactions between people, whereby a verbal community acts in a reinforcing way upon hearing, reading, or otherwise contacting its members' communications.

The irony is again that this is almost the position Lana and others wish to argue for: that social communication is not established and maintained by the (nonsocial) environment but by a verbal community. "Actions need interpretation as to this intentional meaning. In short, people cannot always, and do not expect to, be taken at their word. People expect their actions, particularly the intentional ones, to be interpreted" (p. 106). That is, the communicative language, gestures, or actions are discriminative stimuli for social consequences and not for (nonsocial) environmental consequences (Guerin, 1992). But the important point missed by Lana is that they still can be analyzed into contingencies, with their establishment and maintenance understood as reinforcement effects, so long as one does not attempt to say that communication is reinforced directly by what is specified in the communication.

As an example illustrating the above argument, consider Lana's case of hearing a diagnosed psychotic say, on a bright and sunny day, "It is a dog-bite day" (p. 105). Lana's first point is that the truth of the statement does not refer to the weather. Having no correspondence theory of truth, behavior analysis has no problem with this much. The next clue provided by Lana is that the person's mother reveals that he was bitten by a dog on such a day when he was 6 years old. With a pragmatic version of truth in verbal behavior, this is the very step taken next in analysis, to look for effective reinforcers from a verbal community that might maintain the saying of this sentence. Behavior analysis is still not implying that the psychotic says the sentence *because* he was bitten by a dog, nor that the sunny weather *causes* the psychotic to say the sentence. The position is that it is the effect on a verbal community that maintains saying the sentence, and this might have nothing to do with the dog bite many years ago. The saying of the sentence could also be a discriminative stimulus for all sorts of re-

inforcing social effects that have nothing to do with weather or dogs.

It is Lana, then, who falls foul of his own principles. He writes that if the mother had heard her son's remark, "she would have immediately been able to interpret his remark to *mean* that this was just the kind of day on which he was bitten badly by a vicious dog when he was 6 years old" (p. 105, italics in original). But behavior analysts cannot rest at this point, because a correspondence truth criterion is still being applied here: that the meaning or truth is some sort of correspondence between the remark and the event years earlier. A pragmatic criterion for the truth of verbal behavior actually gets further, I believe, into the hermeneutic analysis by considering that the meaning of the man's remark is the effect it has upon the hearer or a verbal community in general. Far more individual intrepretations of the man's "concrete lived reality" become available with this more detailed contingency analysis.

The conclusion is that behavior analysis might be more consistently hermeneutic than the hermeneutics described by Lana, because it gives the verbal community more control over verbal behavior by not assuming any necessary correspondence between verbal behavior and the nonsocial environment that is specified verbally. Absolute fictions can be generated and maintained by verbal communities because the only "truth" or "meaning" of such fictions are in the effects upon a verbal community. Whether the psychotic man was bitten by a dog when young might have nothing to do with the effects maintaining his verbal behavior.

There is still one more point to make about the verbal community and understanding its products. This is that the reinforcement from a verbal community is usually intermittent and generalized (Guerin, 1992, in press; Skinner, 1957) and takes a long time to structure an individual's verbal behavior (Moerk, 1990). This has the effect that generalized social consequences are not obvious to an observer, so their role in social behavior, and social knowledge in particular, can easily be missed by the brief episodes of observation in the typical experiments of social psychology (cf. Moscovici & Hewstone, 1983). Generalized compliance and generalized verbal behavior are the mainstay of social psychology, and the role of verbal community participation in maintaining these is missed in most social psychological analyses. This is clear, for example, in the word "information," which is used to describe verbal behavior that seems to have no maintaining consequences. "I told her about the Aztecs and she took in all of the information." The role of generalized compliance in a verbal community is missed here, until the question arises of why she would attend, make covert verbal behavior, and possibly later overt behavior changes after listening to the speaker. We can assume that all these activities occur because unobvious social consequences maintain them—verbal communities generally reinforce listening and punish not listening.

Similarly, behavior is generally reinforced by verbal/social communities when it complies with those in authority; effects of this are easily shown (Milgram, 1974). And because the role of generalized community reinforcement is missed in these analyses, the stimulus setting appears to be the major determinant of the behavior. Merely by changing the setting, we can produce more or less compliance, and consequences are not recognized as entering into the equation.

These points mean that the hermeneutic enterprise is really about going to the verbal community to find out the meaning of verbal behavior rather than to the events specified, or corresponding to, the verbal behavior. This turns out to be integral to the analysis of verbal behavior (Skinner, 1957), but without a correspondence criterion for truth. Lana is still correct, though, that behavior analysts need to study this more closely than they have done in the past.

Rhetoric. If the above is true, and verbal behavior can seem to go beyond the environment because the verbal community can support making counterfactual statements, then changing verbal behavior is no longer about changing easy consequences by giving money or food to individuals. Changes to community-maintained verbal behavior must occur by changing the community's patterns of reinforcement, by removing a person from a community, or by providing new sources of reinforcement for new verbal behavior.

Once this is understood, it is easy to see why rhetoric enters into Lana's arguments as an alternative method of historicosocial analysis. The "many possible views of the world" exist as patterns of knowing that are

maintained by a verbal community. Changes in such verbal views presumably must lie within the verbal practices themselves. It follows that persuasion predicted simply on accurate tacting of the environment is often unlikely to be effective. On the other hand, rhetoric deals with changing verbal behavior by using verbal behavior, of attempting "to reach the truth through public discourse" (p. 110). Thus, it should be no surprise that rhetoric now figures as a hermeneutical approach in social psychology (Billig, 1991).

"Because people construct social reality in a way that they do not construct physical reality, the technique used to explain this social reality is hermeneutic and rhetorical rather than scientific and experimental" (p. 113). This, to a behavior analyst, means that *knowing how* involves different contingencies from *knowing that,* so we must concentrate on nonsocial environmental consequences with the former and social (probably generalized) consequences spread over a verbal community with that latter. Behavior analysis is in a position to investigate both experimentally by functional intervention (Sidman, 1960).

THE EXPERIMENTAL ANALYSIS OF SOCIAL BEHAVIOR

Although the present review has rejected Lana's arguments that social behavior cannot be exhaustively studied in an experimental science, the problem still remains that the experimental analysis of social and verbal behavior must be conducted differently than it has been in the past. As Lana points out, social psychologists are now studying social phenomena that have been missed in the past. Some behavior analysts mention that social consequences play a role in verbal control of behavior (Torgrud & Holborn, 1990, p. 290), but these consequences, and their effects, have not yet been systematically examined.

Experimental analyses of social behavior have been undertaken by a few, such as Hake (Hake, Donaldson, & Hyten, 1983; Hake & Olvera, 1978; Hake, Olvera, & Bell, 1975; Hake, Vukelich, & Olvera, 1975; Vukelich & Hake, 1980), Schmitt (1987), Lindsley (1966), and others (Emurian, Emurian, & Brady, 1985; Spradlin, 1985). This work is important, but, as we have seen, the lesson from Lana is that much of social behavior is enacted through arbitrary verbal and gestural patterns that are reinforced through nonspecific reinforcement from verbal communities rather than through the gain or loss of points or through other *specific* social reinforcers. As noted by Skinner (1957, p. 2), the study of social and verbal behavior has many properties (not principles) not shared with other behavior. Verbal behavior that does not correspond to the physical world at all can still be maintained through internally consistent verbal communities. It is likely that new methods of studying such behavior are needed, and new methods of changing such community-maintained verbal behavior need to be considered.

Even studies with nonhumans can address some of these issues, but such work will have to focus on behavior maintained by generalized social reinforcers if useful comparisons are to be made. Until this is done, much of the experimental analysis of social behavior will continue to be irrelevant to social psychologists, because the specific reinforcers used are unlike the consequences that maintain most of our everyday social behavior. Until these "social realities" and social representations (Guerin, in press; Moscovici, 1988; Moscovici & Hewstone, 1983) are dealt with, social psychologists will continue to ignore social behavior analysis.

It is also apparent that the distinction between verbal and nonverbal behavior needs to be made clearly and thoroughly. The special properties of verbal behavior play an important role in producing the misunderstandings between cognitive social psychology and behavior analysis. If behavior analysts concentrate only on specific, tangible reinforcers, then cognitive social psychologists will continue to believe that social knowledge, information, schema, grammar, and other patterns of behavior maintained by a verbal community are not amenable to a behavioral analysis.

The real confusion with cognitive social psychology is more than this, however. The phenomena that are clustered under the label of cognitions usually involve (a) unobservable (covert) behavior, (b) a history of reinforcement, (c) nonspecific consequences from a verbal community, and (d) verbal behavior such as autoclitics. All of these properties lead psychologists to assume either that the contingency control of cognition *does not exist* because it is not obvious, or that the con-

tingency control of cognition *obviously exists*, but only within the head of the individual. Although cognitivist psychology usually conflates the first two, cognitivist social psychology is prey to all confusions. Social psychology therefore makes many motivational assumptions that allow behavior to occur with no clearly defined stimulus control or consequences. For example, in cognitive psychology subjects *always* seem to process the information they are given by the experimenter—attentional selections seem to be mostly independent of external, social contingencies. This again follows from the confusions listed above. As noted at the beginning of this paper, the motivational sources in social psychology are currently assigned to affect or the self.

Lana concludes his book by noting that cognitive social psychology has moved to become more descriptive than predictive, which he sees as fitting in with the trend towards hermeneutics and historicosocial analysis. From the behavior-analytic point of view, however, a description will be predictive only because nonspecific community reinforcers are maintaining the behavior that is being interpreted. These are overlooked, so the described stimulus setting seems to be the only variable controlling that behavior. Therefore, to bring sociality back into the descriptions of cognitive and social structures seems to require a special hermeneutic social understanding or social schema. This is the point of Lana's book.

Thus, we have to thank Lana not only for supporting behavior analysis as a viable alternative in social psychology, but also for showing us that there are social phenomena that we need to analyze further if social psychologists are to pay any attention. When behavior analysts have supplied more extensive experimental and conceptual analyses of verbal community reinforcement, and can demonstrate the roles of nonspecific social consequences in maintaining much of our social behavior and cognition, then social psychology might take more interest in behavior analysis.

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